Selection or Specialization?
The Impact of Legal Marriage on Adult Earnings in Sweden

by

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Abstract:

A positive male marriage premium has been found in many studies. The source of the marriage premium is unclear and debated – does it result from selection or specialization? Our paper analyzes earnings for married and long-term cohabiting Swedish men and women.   We use data on the earnings in 1985 and 1990 of married and cohabiting parents of a random sample of about 120,000 children born in 1974-87. We employ a number of empirical strategies to identify the causal effect of marriage. In particular, we use a natural experiment, the marriage boom in Sweden in 1989, created by the reform of the widow's pension system to control for the selection of marriage and identify the causal effect of marriage. Our results show that all of the male marriage premium can be explained by positive selection whereas the female marriage penalty is explained by increased specialization in home production and childcare. When we examine the effect of marriage on total family earnings, we find evidence that the positive selection of men into marriage leads to the increased specialization of women.

1. Introduction

Many studies have found a marriage premium for men. The source of the premium is however unclear and debated (see Ribar 2004 for a review) – does it come from specialization or selection? Marriage may be conducive to a gender-based division of labor where men specialize in human capital investments which raise their market productivity and women specialize in non-market production. The male marriage premium may, on the other hand, reflect differences in men's productivity which are unobserved for researchers but valued both by employers and prospective wives. In fact, most studies that control for selectivity find that selection does explain part of the male marriage premium, estimates vary from 50 percent (Stratton 2002) to 10 percent (Ginther & Zavodny 2001). If most of the marriage premium is not explained by selection, the increase in male wages after marriage may result from specialization. A third explanation posits employer bias in favor of married employees. However, this explanation is more difficult to test than either the selection or specialization hypotheses. Furthermore, very few studies have explored the existence of a marriage premium among women (see however Waldfogel 1997, Budig & England 2001). If gender-based specialization explains the lion's share of the male marriage premium we should expect married women to have lower wages than unmarried women. This paper examines the causal effect of marriage on men's and women's earnings and finds that the male marriage premium is largely explained by selection whereas the female marriage penalty results from increased specialization.

Our paper analyzes earnings for married and cohabiting Swedish men and women who have children. Sweden is interesting because it has a high prevalence of cohabitation among couples with children. In addition, since the 1960s the Swedish government has enacted reforms intended to promote gender equality in the labor market and in the home. Few studies have compared the marriage and the cohabitation wage premiums, but Stratton (2002) found a lower wage premium for

cohabitating men than for married men in the US and Richardson (2002) obtained a similar result for Sweden. Marriage may lead to a greater specialization than does cohabitation because *inter alia* its expected duration is longer and the legal arrangements provide for pooling of resources to a higher degree. This greater specialization is expected to result in higher male productivity in the labor market. If this is the case, we should expect wages for cohabiting women to be higher than for married women.

We use data on annual earnings in 1985 and 1990 of men and women who were both biological parents of a random sample of about 120,000 children born in Sweden in 1974-87. Parents were born in and lived in Sweden in 1985 and 1990 and either married or cohabiting. The purpose of using data on parents is to compare homogenous groups-- we want to compare long-term cohabitants to married individuals. In addition, this approach allows us to use the age of the oldest common child as a proxy for union duration, information which is otherwise difficult to obtain for cohabitants and for married couples who cohabited prior to marrying.

However, identifying the causal effect of marriage on outcomes is complicated by the selection problem. In this case, it could be that men who have higher wages or who are expecting a faster wage growth are more likely to marry. For this reason, we use a natural experiment to control for selection into marriage. The experiment is the marriage boom in Sweden in the last two months of 1989, created by the reform of the widow's pension system from January 1990. In 1988, when the Swedish parliament enacted a reform abolishing the widow's pension starting in January 1990, it included transitional provisions that allowed women who were born before 1945 and married by the end of 1989 to be entitled to a widow's pension if their husband died. Those already receiving a widow's pension would continue to do so as long as they lived. The implications became gradually known to Swedish public and resulted in a dramatic marriage boom in the last two months of 1989.

We use the change in the widow's pension system to assess the causal effect of marriage, since marriage in the end of 1989 can be assumed to be less correlated with previous earnings than other marriages. Thus, our model allows us to compare the change in earnings for men and women who married in November and December 1989 to those of men and women who continued to cohabitate or were married earlier. Our results show that all of the male marriage premium can be explained by positive selection whereas the female marriage penalty is explained by increased specialization in home production and childcare. When we examine the effect of marriage on total family earnings, we find evidence that the positive selection of men into marriage leads to the increased specialization of women.

The paper proceeds as follows: Section 2 describes the trends in cohabitation and marriage, and discusses the legal differences between marriage and cohabitation in Sweden. Section 3 presents our theoretical perspectives. In Section 4 we present our data, describe the marriage boom in the end of 1989 and discuss our estimation method. Section 5 presents our findings. We end with a discussion of the results.

2. Cohabitation and marriage in Sweden

2.1 Trends in cohabitation and marriage in Sweden

Cohabiting unions are more common in Sweden than anywhere else in the industrialized world, although levels in Denmark now come rather close. Marriage rates have been declining since the late 1960s while cohabitation rates have been rising. At the same time, the duration of cohabitation has increased. For example, among women born in the late 1940s about half had married their partner after three years of cohabitation while this was the case for only about one-tenth of women born in the

late 1960s – after five years of cohabitation about two-thirds and one-third of the respective cohorts had married (Bracher and Santow 1998).

Thus, cohabitations in Sweden are stable and relatively long-lasting unions which may afford the same benefits as marriage. They are, however, less stable than formal marriages, and break-up rates have increased over cohorts. For example, about one-tenth of the first consensual unions for women born in the late 1940s were dissolved within three years, while this was true for about one-fourth of the first unions for women born in the mid-1960s (Hoem B. 1995). Analyzing dissolution risks among cohabiting and married women and men, Bracher and Santow (2001) found that cohabitants faced higher dissolutions risks net of factors such as presence and age of children, work history, income and union duration, which may suggest the presence of unobserved heterogeneity. Despite of elevated marriage rates for pregnant cohabiting women, the majority of women are not formally married at first birth, but cohabiting in Sweden. Births to non-cohabiting, unmarried women are rare (less than 10 percent of all births). Sweden is probably unique in the industrialized world in having a lower median age for women at first birth than at first marriage; both medians have been increasing, the former from 25.0 years in 1980 to 26.2 years in 1993 and to 28.4 years in 2001 and the latter from 25.6 years to 27.4 years and to 29.6 years in the same years.

2.2 Legal differences between cohabitation and marriage in Sweden in 1989[1]

It is commonly believed that there are very minor differences in the legal implications of marriage and cohabitation in Sweden. However, this is only true as long as the union stays intact, if couple has no children together (or prior to their union), or if they have no savings or property. These differences are summarized in Table 1. A crucial difference between married spouses and cohabitants is that married spouses are obliged under the law to support each other according to their ability. Further, for

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[1] This Section draws on Agell (1982, 1989), Insulander-Lindh & Thunberg (1996) and Ståhlberg (2004).

a child of married parents, paternity is automatically attributed to the husband of the mother and the couple will have joint custody of the child. However, if the parents are unmarried or cohabiting, the father has to acknowledge paternity, and they will have joint custody of the child provided they both agree on that, which most couples do. While earnings of married couples have been taxed individually since 1971, wealth and income from property and businesses are taxed jointly. Cohabitants are taxed jointly on wealth and property income only if they have children under age 18 together or if they have previously been married to each other.

Moreover, in a consensual union there is no community property as there is in marriage. The 1988 "cohabitation-law" stipulates that if cohabitants split-up, what they have acquired for common use should be divided between them. This applies to dwellings provided they have been acquired for common use. In the event of a separation, according to the law, the partner who is most in need of the apartment/house should have it, regardless of who bought it.[2] Private property, such as stock and bank savings, is not divided. This is true also for property that was acquired before cohabitation and for property that has been acquired for private use.

Finally, cohabiting couples do not automatically inherit each other. Cohabiting couples may write testaments in favor of each other, but bequests are taxed. Survivors from a cohabiting union have never been entitled to widows' or widowers' pension in the supplementary pension system, but under certain very specific circumstances, they were eligible in the general retirement scheme.[3] Those who received a widow's/widower's pension prior to 1990 and those who were eligible under the pre-1990 rules still receive their pensions and will do so as long as they live. There continues to be widow's/widower's pensions available from collective bargaining agreements, however, the

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[2] However, if the house/apartment was bought by one of the partners, the other one has to buy the owner off.
[3] In the new 1990 pension system there is a general 'readjustment' pension for survivors who have children under 12, regardless of whether they were married or cohabiting. If there are no children the adjustment pension is only available for ten months.

availability and size of such pensions differ across agreements. Thus, blue-collar workers and low-earning white-collar workers in the private sector have no such protection for their survivors. In contrast, widows/widowers of high-earning white collar workers in the private sector, receive survivor's pensions as long as they live and as long as they do not remarry. Thus, these legal implications should affect the incentives to marry differently for different groups. We should expect the selection into marriage and cohabitation to be non-random processes, and as a result, married and cohabiting parents should differ.[4]

3. Related literature

Unlike cohabitation which ends when one partner moves out, marriage requires a legal separation of property and custody rights, making it more difficult to dissolve. Thus, it could be that marriage is a signal of greater commitment. Consequently, the expected duration of a marriage is longer than that of a consensual union. All these aspects may provide for a higher degree of pooling of family resources and a greater degree of specialization within the family leading to economies of scale in household production among married couples. A traditional, gender-based division of labor would thus allow married men more than cohabiting men to specialize in market work, while married women would be more specialized in household production than cohabiting women. This is indicated by the fact that among employed mothers of children below age 10 the fraction working part time was 62 percent among married mothers but only 35 percent among cohabiting mothers in 1990 (Swedish Level of Living Survey 1991).[5]

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[4] Henz and Sundström (2001) show, for example, that married mothers were more highly educated and older at first birth, on average, than cohabiting mothers. The differences between the two groups have increased over time.
[5] In 1999 (outside our study period) these differences had been reduced; 46 percent of married mothers worked part time compared to 41 percent among cohabiting mothers. We are grateful to Elin Olsson for help with these computations.

The literature on the U.S. male marriage premium is vast and contradictory. Korenman and Neumark (1991) find little evidence that selection into marriage biases estimates of the marriage premium after controlling for fixed effects. Whereas Cornwell and Rupert (1997), in contrast, conclude that selection underlies much of the marriage premium using the same approach. Gray (1997) uses data two sample time periods to examine the change male marriage premium. Like Blackburn and Korenman (1994), Gray reports a significant decrease in the marriage premium over time. Taken together, these studies do not reach a consensus on the size of the marriage premium or its underlying causes.

Ginther and Zavodny (2001) analyze the marriage premium among US white males using a very different approach to controlling for selection. They utilize data on 'shotgun' weddings (marriage due to a pre-marital conception) to control for selection since such weddings are assumed to be less correlated with earnings ability. They find that men who had a 'shotgun' wedding earn about 15 percent more than never married men, while men who married without a pre-marital conception earned about 16 percent more than never married men. Their result thus suggests that at most 10 percent of the marriage premium is due to selection.

Few studies have compared the married and cohabiting male wage premiums. One of the few that exists is Stratton (2002) who finds that married white men in the U.S. earn higher wages than cohabiting white men, who in turn earn more than single men. Using fixed-effect models to control for selectivity she finds that the wage premium for married men is reduced by half and the premium for cohabiting men disappears completely. The higher wages for married men is primarily attributed to faster wage growth, i.e., increased market productivity, during marriage. Only the cohabiting men in long-lasting unions experience wage gains that match those of married men.

Analyzing the marriage premium for Swedish males over the period 1968-1991, Richardson (2002) finds that in 1968 married men earned about 23 percent more than never married men while cohabiting men earned about two-thirds of married men.[6] In 1991 the premium for married men had declined to 8 percent and to about half of that (4 percent) for cohabiting men. Using a JMP-decomposition she demonstrates that the decline in the premium mainly reflects diminishing productivity differences between married and never-married men, i.e., lower degree of specialization. Consequently, longer marriage duration was not associated with a higher marriage premium. Estimating a fixed-effect model to control for unobserved heterogeneity she finds a statistically significant premium of about 10 percent for married men for the whole period and one of about 5 percent for cohabiting men. Thus, although specialization accounts for a greater part of the premium in the late sixties and early seventies, both specialization and selection contribute to the premium in the later part of the period studied.

Using register data on about 35,000 young Danish men over the years 1984-2000, Datta Gupta, Smith & Stratton (2005) find a marriage premium of about 4 percent which is reduced to 2 percent after controlling for selectivity. The cohabitation wage premium is of the same size. They, further, find that part of the marriage premium is in fact a 'fatherhood' premium-- men receive a wage premium during their first years as fathers.

As mentioned, the possible marriage premium for women has attracted little research attention and the few studies that exist have mainly been aimed at assessing the impact of children on women's wages. Analyzing U.S. data over the years 1968-88 for white, black and Hispanic women, Waldfogel (1997) finds, however, a small positive impact of marriage on women's wages. Her fixed effects estimates show that married women have higher wages than never married women but lower wages

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[6] The results are obtained from seemingly unrelated equations controlling for age and its square, years of schooling, work experience and its square, working conditions and number of children 0- 19 years in the household.

than divorced women. In line with her results Budig and England (2001) find marriage to be associated with higher earnings for women using U.S. data for the years 1982-1993. This was true also in the fixed effects model. Divorced women, however, earned about as much as those married.

4. Data and methods

4.1 Data and sample

We use data on earnings in 1985 and 1990 of the biological parents of a random sample of about 120,000 children born in 1978-87 (drawn from the population registers of Statistics Sweden). The parents were born in Sweden and were married or cohabiting with each other in both 1985 and 1990. Additional information was obtained from the 1985 and 1990 censuses and from Statistics Sweden special multigenerational register, allowing us to create parents' marital history. Annual earnings are obtained from the tax records. To obtain estimates as close as possible to those for hourly wages (not available), we restrict the analysis to earnings above SEK 100,000 in 1990 prices both years and for both women and men.[7] Our explanatory variables include educational attainment, and number of children living in the household. We use age of the oldest common biological child as a proxy for union duration, for both married and cohabiting parents as most Swedes cohabit before marriage. We present means and frequencies for our samples in Table 2a and 2b.

Clearly, there are large differences between married fathers (mothers) and cohabiting fathers (mothers) both years. Married persons have higher earnings and higher education, they are older, have more children, and their union has lasted longer. We also observe the large number of persons marrying in Nov-Dec 1989. As expected, the marriage boom contributed to make the married in 1990

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[7] This, in fact, the same earnings limit as Antelius and Björklund (2000) apply when they demonstrate (for both sexes in 1991) that this limit produces estimates on returns to education that are highly similar to those obtained using hourly earnings.

more similar to those cohabiting in 1985 and, for example, reducing the fraction with a university education among married mothers in 1990.

4.2 The Swedish widow's pension reform and the marriage boom in 1989

In 1988 the Swedish parliament enacted a reform abolishing the Widow's Pension beginning in January 1990.   Under the old system, if a woman's husband (and certain cohabiting partners) died she was entitled to a widow's pension for the rest of her life. The pension was based on the husband's retirement income. A widow who was below the general retirement age of 65 received 40 percent of his retirement income; starting at age 65 she received the difference between the Widow's Pension and her own pension. This system was replaced in 1990 by a system where children of the deceased receive Child Pensions until age 18 and the surviving partner — both sexes, married or cohabiting — receive an Adjustment Pension for up to 12 months.

The Adjustment Pension depends upon the age of the children and the income of deceased. Thus, it is not an unconditional right like the Widow's Pension. Survivors receive the Adjustment Pension as long as they have children below age 12. For example, if the husband died in 1990 and the couple had a 16-year-old child, the child would receive a Child Pension until age 18, and the widow would get the Adjustment Pension for a maximum of 12 months. Survivors who have no children at home could get the Adjustment Pension for a maximum of 10 months. In sum, the Adjustment Pension is only available for about one year or until the youngest child turns 12, whereas the Widow's Pension was for life.   The change in the Swedish Widow's Pension was particularly disadvantageous for women with older children or no children at home.

Widows who received a pension prior to 1990 and those who were eligible under the pre-1990 rules receive their Widow's Pensions and will do so as long as they live. Importantly for our analysis,

there were a number of transitional provisions included in the reform, the main impact of these being that **all** non-married women born before 1945 could gain rights to the Swedish Widow's Pension by marrying before the end of 1989 (Hoem 1991). In addition, some never-married women who were born after 1945 and had a child could improve their rights to the Widow's Pension by marrying the child's father before 1990, but the entitlement was more restrictive than for older women.

The effect of the policy change was dramatic. The propensity to marry sky-rocketed in December 1989, especially for cohabiting women; the number of marriages increased from an average of 3,000 in previous Decembers to 64,000 in December, 1989, a 21-fold increase (Andersson 1998, Hoem 1991). Figure 1 reproduces results from Andersson (2003), showing the impact of the change in the Swedish Widow's Pension on marriage rates in 1989. Although marriage rates in November and December 1989 were particularly elevated for women over 45 (Hoem 1991), they were also very high for younger women, who would not benefit directly from marrying. We can interpret the latter change as a "bandwagon" effect, that is, couples who held more or less vague plans of marrying in the future, stopped putting it off and married because so many other couples were doing so. Alternatively, they may have found it too time consuming to find out whether the woman would be eligible for a widow's pension and simpler to just to marry. Still another interpretation of the "bandwagon" effect is that the marriage boom made it less expensive to marry since it became acceptable to marry without having a costly party.[8] This dramatic response to the change in Sweden's Widow's Pension system constitutes a quasi-natural experiment that will enable us to examine the causal effect of marriage on child and adult outcomes in Sweden.

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[8] The most common answer among cohabiting women to the question of why they were not planning to marry was that they could not afford the wedding they wished to have (Hoem 1995).

Figure 1: Annual index of marriage-risk level.
Never-married Swedish women, 1971-2002,
standardized for parity and age.

Risks relative to 1980
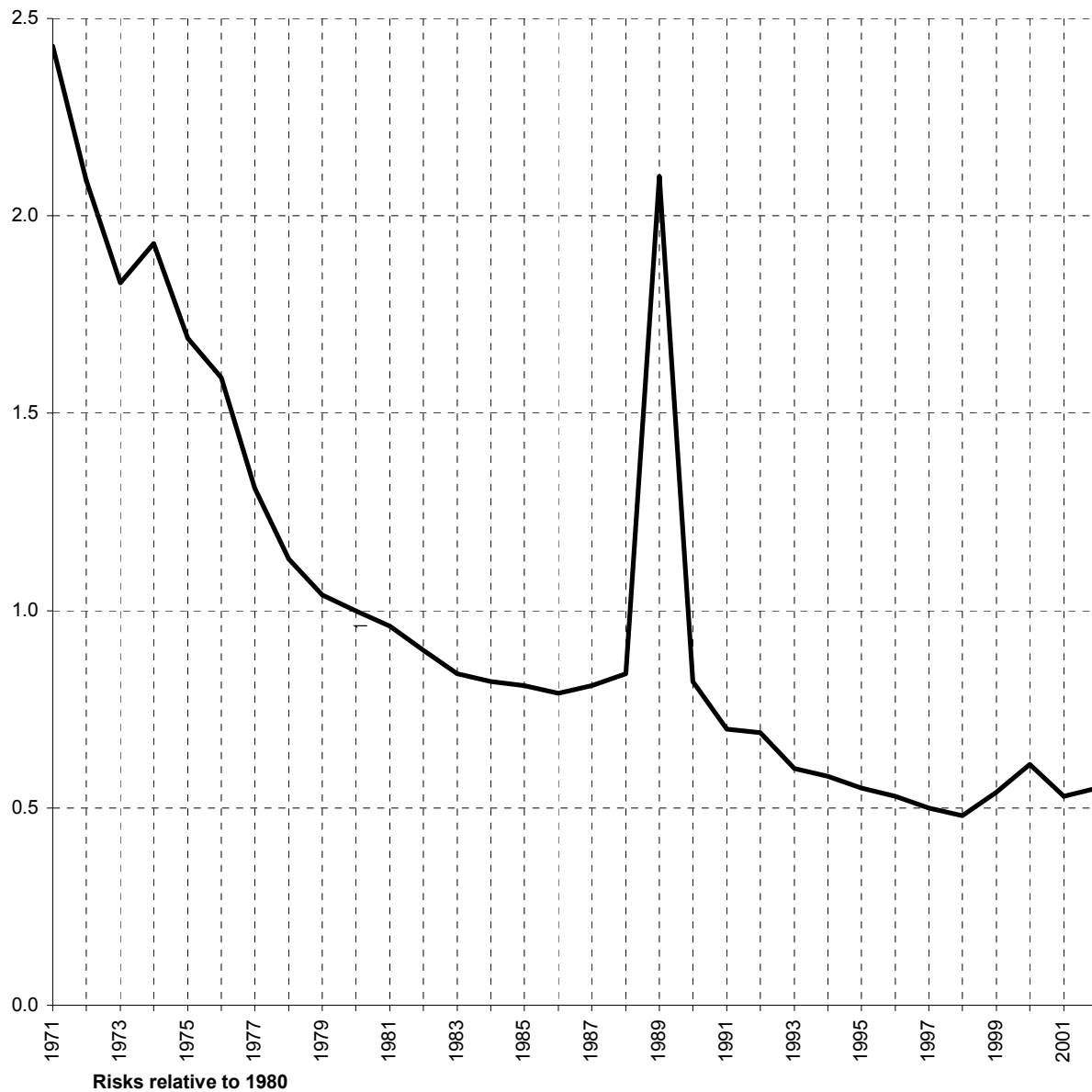
Source: Gunnar Andersson 2003: "Demographic trends in Sweden: An update of childbearing and nuptiality through 2002". MPIDR Working Paper, WP 2003-034. Max Planck Institute for Demographic Research, Rostock.

4.3 Estimation methods

We begin with simple cross-sectional estimates of the effect of marriage on earnings. However, as this approach does not control for self-selection of marriage we next implement three different methods to control for selection.

Our first approach to addressing selection in marriage assumes that marriage is not randomly assigned and uses a quasi-natural experiment to identify the causal effect of marriage on parents' earnings. A quasi-natural experiment occurs when a subpopulation is affected by a natural event or policy change that causes exogenous change in a variable usually endogenously chosen. Ginther and Zavodny (2001) use shotgun weddings as a quasi-natural experiment to identify the effect of the marriage premium. In this paper, the propensity to marry was affected by the change in the Swedish Widow's Pension creating a quasi-natural experiment.

There are several benefits to using the quasi-natural experiment to identify the causal effect of marriage on the marriage premium. First, the Swedish Widow's Pension natural experiment allows the researcher to evaluate the role of marriage in isolation, holding other variables that affect the likelihood of marriage constant. Second, the marriage boom at the end of 1989 generated two treatment groups (those who were eligible for the Widow's Pension and married and those who were not eligible but 'jumped on the marriage bandwagon') along with two control groups (those who were eligible for the Widow's Pension and did not marry and those who were not eligible and did not marry). Our analysis will compare and contrast the impact of marriage and cohabitation using these treatment and control groups in order to yield valuable empirical insights.
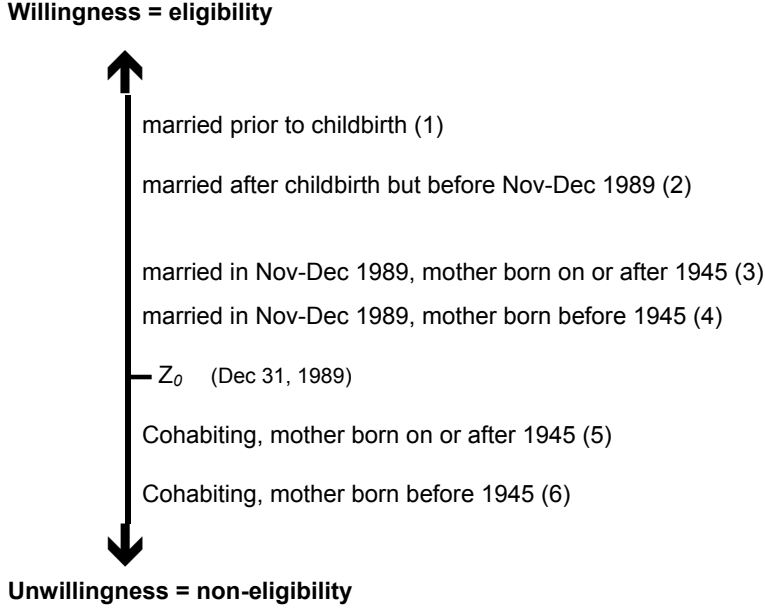
To fix ideas, consider the simple regression model given in (1):

$$y = \alpha + \beta M + u \tag{1}$$

where y is male (female) earnings, M is marital status, the endogenous treatment that is correlated with the error term, u. Let there be a policy intervention — the change in the Swedish Widow's Pension — that affects the $\Pr[M_i = 1 \,|\, x]$. We can evaluate the impact of the quasi-natural experiment by comparing those affected by the change in the Swedish widow's pension with non-impacted groups post-intervention. Let z_i be an indicator variable of eligibility for widow's pension which is discontinuous in calendar time, namely at $z_0 =$ December 31, 1989 (see Figure 2 below). Let $M_i = f(z)$ where $\Pr[M_i = 1 \,|\, z_i = z]$ is known to be discontinuous at the point z_0. Treatment assignment is determined not only by z_i but by additional variables, some of which may be unobserved. Thus, the change in the widow's pension system allows us to examine the marginal effect of marriage on outcomes.

To estimate the effect of treatment (marriage) we compare the outcomes of individuals with an incremental increased exposure to marriage (just above the threshold, z_0) to the outcome of those without such an increased exposure (just below z_0), given by Figure 2 below. The dummy variables include indicators for couples who (1) married before the birth of their child (omitted category); (2) married after child's birth and before 1989; (3) married in 1989 and ineligible for the Widow's Pension; (4) married in 1989 and eligible for the Widow's Pension; (5) cohabiting after 1989 and ineligible for the Widow's Pension. (6) cohabiting after 1989 and eligible for the Widow's Pension; Thus, we may compare the outcomes of adults who married in late 1989 and had a financial incentive to do so, where the mother was born before 1945 (Group 4 in Figure 2)-- those who were affected by the policy change--to the outcomes of adults who remained cohabiting and had no financial incentive to marry, where the mother was born on or after 1945 (Group 5).

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Figure 2: Parents' willingness to marry and eligibility for widow's pension

Willingness = eligibility

married prior to childbirth (1)

married after childbirth but before Nov-Dec 1989 (2)

married in Nov-Dec 1989, mother born on or after 1945 (3)
married in Nov-Dec 1989, mother born before 1945 (4)

Z_0 (Dec 31, 1989)

Cohabiting, mother born on or after 1945 (5)

Cohabiting, mother born before 1945 (6)

Unwillingness = non-eligibility

We implement this strategy by including a series of marital status dummy variables K in place of M_i and including additional covariates X, we estimate the following equation:

$$y_i = \alpha + \phi K_i + X_i{}'\beta + u_i \qquad (2)$$

Conceptually, this approach is equivalent to calculating mean differences in the impact of marriage for these different groups controlling for observable characteristics. These marital status dummies account for unobserved differences in characteristics shared by these different groups.

The natural experiment approach also has several limitations. Choice of treatment and control groups affect the reliability of the estimates. Thus, we make a number of comparisons. Those women eligible for the Swedish widow's pension were older and often had children from previous unions (Hoem 1991). Thus, the causal impact of marriage derived from these estimates of the treated may not generalize to the population of Sweden as a whole. However, it is important to note that any response to a policy promoting marriage will induce a select group to change its behavior. Thus, if

there is a positive impact of marriage in response to the change in the widow's pension, this would provide strong evidence of the causal effect of marriage.

Our second approach is to examine how changes from cohabitation to marriage affect the marriage premium using fixed effects methods. We consider fixed effects models because they allow us to control for unobserved, individual- or family-specific factors that do not vary over time and may be correlated with the marriage decision and earnings. Formally, let y_{it} measure male (female) earnings, where i indexes individuals and t indexes time. Let X_{it} be characteristics that vary across individuals and time, and M_{it} be marital status. Consider the linear model:

$$y_{it} = \beta X_{it} + \gamma M_{it} + \varepsilon_{it}.$$
(3)

We can decompose the error term into two components: $\varepsilon_{it} = \eta_i + \upsilon_{it}$, where η_i is the individual-specific component, and υ_{it} is random error. If η_i is correlated with marital status and υ_{it} is uncorrelated with it, then first differencing will eliminate selection bias. We can difference (3) with respect to time and estimate the following equation:

$$\Delta y_i = \beta \Delta X_i + \gamma \Delta M_i + \Delta \varepsilon_i.$$
(4)

Under our assumptions, this procedure eliminates any observed or unobserved variables that do not vary by individual.

Our third approach is to estimate difference-in-difference models comparing both groups of fathers and mothers who married in Nov-Dec 1989, (Groups 3 plus 4) to those who continued to cohabit and were not eligible for widow's pension (Group 5). We use this approach because marriages that occurred as a result of the widow's pension reforms can be assumed to be less influenced by prior earnings growth than other marriages between 1985 and 1990.

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5. Findings

5.1 Cross-section estimates

We start by estimating cross-section regressions for fathers in 1985 and find that married fathers earn about four percent more than cohabiting fathers (Table 3a, Model 1). Even though marriage and cohabitation are quite similar in Sweden, legal marriage has an impact on earnings. The estimates in Model 2 indicate that a small part of the marriage premium can be attributed to the longer duration of marital unions. The number of children is negatively related to earnings. Turning to the cross-section regressions for mothers in 1985, we see that the coefficient for married is virtually zero in Model 1 (Table 3b). However, it becomes positive and (weakly) significant when we control for union duration (Model 2). In contrast to the results for men, union duration is negatively related to mothers' earnings which suggests specialization in home production may reduce women's wages over time. Further, when we take account of the number of children at home, the coefficient for married increases in magnitude and significance but is counterbalanced by the negative coefficient for children (Model 3). Thus, there is a marriage premium for women too, but it is only about one-sixth of that for men and is reduced by union duration and number of children. In line with this, when we experimented with a lower limit on annual earnings we found that the marriage premium turned into a (statistically significant) marriage 'penalty' which is most likely due to the larger extent of part-time work among married mothers.

Next, we estimate cross-section regressions for fathers and mothers in 1990. The estimates for fathers point to a marriage premium of the same magnitude as that found for 1985 (Table 4a, Model 1). However, when we disaggregate the married by time of marriage, we see that those who married before the birth of the focal child (i.e., Group 1) receive the largest marriage premium (Model 2-4). Also, fathers in Group 2 have higher earnings than those in Group 3 and the remaining groups (Model

4). (Equality of the coefficients for Group 2 and 3 was rejected by an F-test at the 1 %-level). Group 4 contains those individuals who have a financial incentive to marry as a result of the widow's pension. These men earn a lower marriage premium than those in Groups 1- 3. Similar to the results for 1985 union duration is seen be to positively associated with male earnings while number of children is negatively correlated, and more so than in 1985.

Turning to the cross-section estimates for mothers in 1990 in Table 4b, we find, as for 1985, a zero marriage premium in Model 1. Next, we see a similar pattern as that found for fathers (Model 2-4): mothers who were married before the birth of the focal child (Group 1) earn most, those in Group 2 and 5 earn almost as much, and those in Group 4 earn the least while at the same time having the greatest financial incentive to marry (equality of coefficients for Group 2 and 3 was rejected at the 1 %-level and equality of those for Group 4 and 5 was rejected at the 5 %-level). Taken together, the results in Tables 4a and 4b suggest that selection may play a role in the marriage premium. Those who married in response to the change in the Widow's pension had a much lower marriage premium than those who married before the birth of the focal child. We now consider the fixed effects estimates.

5.2 Fixed effects models

Estimating fixed effect models eliminates time constant individual effects and should control for any time-invariant selectivity under the assumption that those who marry in 1985-1990, and who identify the marriage coefficient, are representative of the effect of marriage on earnings. Thus, if the marriage premiums observed in the cross-section analyses are due to selection into marriage based on productivity-related characteristics we should expect to find a smaller or even non-significant coefficient for married persons in the fixed effects models. This is, indeed, what we find: for fathers

we find a positive and significant marriage premium which is less than half of that in the cross-section analysis (Table 5a, Model 1). As is clear from Model 2, most of the premium is 'explained' by a positive and significant impact of an increase in the number of children at home which is very much in line with the findings for Denmark by Datta Gupta et al (2005). For mothers we find a negative and significant impact of marriage on earnings, a marriage penalty (Table 5b, Model). The impact of children is negative and significant; each child reduces mothers' earnings equally as much as marriage does (Model 2).

These findings suggests that selection explains the majority of the marriage premium for fathers. However, the negative effect of marriage on women's earnings is most consistent with the specialization hypothesis for mothers. The implication is that more productive men are selected into marriage and fatherhood, and they are more likely to turn their cohabitation into marriage. In addition, the results show that there is higher degree of gender-based specialization in marriage than in consensual unions which results in lower earnings for married women than for cohabiting women. This result is strengthened when the earnings restriction for women is relaxed--the marriage penalty triples in magnitude for women (results not shown), which reflects the higher frequency of part-time work and non-market activity of married women relative to cohabitating women.

5.3 Difference-in-difference estimates

Marriage during the marriage boom in the end of 1989 can be assumed to be less influenced by productivity-related characteristics than other marriages in that period. Those who married in late 1989 are assumed to be responding to financial incentives to marry and not positive selection. Thus, we estimate difference-in-difference models comparing both groups of fathers and mothers who married in Nov-Dec 1989, that is, Group 3 plus 4, to those who continued to cohabit and were not

eligible for widow's pension (Group 5). The resulting estimates are presented in Table 6a and 6b and have the same signs as the previous fixed-effects estimates, but are much smaller in magnitude and not significant for fathers or mothers. Thus, there is no marriage premium for fathers and no marriage penalty for mothers among those who married in the end of 1989. However, we do observe a statistically significant child premium for men and a large and significant child penalty for women. The effect of children on earnings suggests that marriage still affords some gender-specialization within the household in terms of childcare but this specialization does not translate into positive or negative changes in earnings upon marriage. These findings suggest that the positive selection into marriage based on productivity for fathers and the marriage penalty for mothers seen in cross-sectional estimates of the marriage premium are the result of the gender-division of labor in the home and the labor market. However, the increase in gender-specialization among married couples does not translate into increased labor market productivity for fathers, and does not result in the changes in labor supply observed for women in the fixed effects estimates.

5.3 Effect of Marriage on Family Earnings

As mentioned in the introduction, marriage may allow for more specialization within a relationship. Previous studies have assumed that this specialization would lead to increased productivity for men in the market. However, our results up to now have shown that marriage has little impact on men's labor market productivity while seeming to afford increased specialization in childcare for women. We can examine these in greater detail by looking at the overall impact of marriage on total family earnings—men's and women's earnings combined. Our previous results have limited both partner's earnings to 100,000 SEK which reduces the sample size for women if they choose to work fewer hours. We now relax this restriction when examining the impact of marriage on total family earnings.

Table 7a reports fixed effects estimates of the impact of marriage on family earnings where men's earnings are limited to those earning more than 100,000 SEK and there is no earnings limit imposed on women. Model 1 shows that marriage has a negative and significant impact on total family earnings, reducing it by approximately 17 percent. Model 2 shows that a significant portion of the marriage penalty is actually the impact of children on earnings. Nonetheless, marriage generates a statistically significant 9 percent penalty after controlling for presence of children. Table 7b reports fixed effects estimates of the impact of marriage on family earnings where women's earnings are limited to those earning more than 100,000 SEK and there is no earnings limit imposed on men. Women who earn more than 100,000 SEK in 1985 and 1990 are most likely working full time in both years and married to other full time workers. The sample size drops by 43 percent and the estimated impact of marriage drops significantly. Thus, the negative impact of marriage on family earnings falls by over 75 percent compared to estimates in Table 7a. The negative impact of children on family earnings drops significantly as well. Taken together, these results indicate that upon marriage, women reduce hours of work significantly, especially if they have more children. Total family earnings fall after marriage as women specialize in home production.

Table 8 shows the difference-in-differences estimates for those who married in November-December 1989 using the 100,000 SEK restriction for men's earnings only. Similar to Tables 6a and 6b, marriage has no significant impact on family earnings even after controlling for presence of children. However Model 2 shows that children have a large and negative impact on family earnings. Thus, women in families who respond to financial incentives to marry do not apparently specialize as much as those who choose to marry for other reasons. These results indicate that positive selection into marriage actually enhances specialization within marriage.

6. Conclusions

We examine the impact of marital status on earnings for long-term cohabiting and married men and women with biological children in Sweden. Our study uses a variety of methods to control for selection into marriage. In our cross-sectional analyses we find a marriage premium for both men and women—although the marriage premium for women is only 15 percent of the men's premium. Duration of the union increases the marriage premium for men but decreases it for women. This suggests specialization in household production may affect the marriage premium. However, when we control for the timing of marriage in 1989, we find evidence that selection matters. Those who married in 1989 for financial reasons had a much lower marriage premium than those who married before the birth of the focal child.

When we use fixed-effects models to control for selection into marriage we find support for the selection hypothesis for fathers: the marriage premium for fathers is small and less than half of that found in the cross-section analysis, but remains statistically significant. For mothers we find a statistically significant marriage penalty which is consistent with the specialization hypothesis. Although cohabitation and marriage are quite similar in Sweden, we find that marriage has a significant impact on earnings.

Next, we use the change in the widow's pension and resulting marriage boom in 1989 as a natural experiment to take account of possible selection using a difference-in-differences approach. Many who married in November/December 1989 had a financial incentive to do so, although a number of people married who were not eligible for the widow's pension. The resulting estimates show no marriage premium for fathers and no marriage penalty for mothers. However, we do observe a significant child premium for fathers and a significant child penalty for mothers.

Finally, we examine the effect of marriage on total family earnings using fixed effects and difference-in-differences estimation. Our results show that marriage decreases family earnings and this decrease can largely be attributed to a reduction in the labor supply of women after marriage. Children also reduce family earnings. However, those who married for financial reasons in 1989 suffer no marriage penalty but do experience a significant child penalty on earnings.

Taken together our results suggest that men are positively selected into marriage. The story for women is more nuanced. Cross-sectional estimates of the effect of marriage on female earnings are either zero or negative. Using fixed effects methods to control for selection increases the size and significance of the negative effect of marriage on female earnings. However, women who respond to financial incentives and marry experience no statistically significant impact of marriage on earnings but still experience a child penalty. The positive selection of men into marriage enhances and encourages increased specialization by women in household production and childcare. However, women's specialization does not seem to translate into enhanced market productivity for men. Thus, positive selection leads to increased specialization within the marriage with little or no spillover on market productivity.

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Table 1: Differences in Legal Arrangement of Marriage and Cohabitation in Sweden

Legal Arrangement:	Marriage:	Cohabitation:
Obligation to Support Spouse	• Yes: Spouses obligated to support one another	• No: Partners not obligated to support one another
Paternity	• Husband of mother is granted paternity	• Biological father must legally recognize child
Custody	• Joint Custody	• Requires agreement by parents
Taxation	• Property Income and Wealth Taxed Jointly	• Property Income and Wealth Taxed Jointly only if couple shares children < 18 years
Community Property	• Yes	• No
Inheritance	• Spouses automatically inherit	• Written testament required for inheritance
	• Inheritance not taxed	• Inheritance taxed
Dissolution Costs	• Dissolution requires legal costs	• Limited or no legal costs

Table 2a. Means and frequencies for the male samples[a] for 1985 and 1990 by marital status.

	Fathers 1985			Fathers 1990		
	Married	Cohabiting	All	Married	Cohabiting	All
Log earnings[b]	7.14 (.298)	7.02 (.230)	7.11 (.288)	7.61 (.323)	7.51 (.278)	7.60 (.319)
Age	35.0	31.5	34.1	38.8	35.8	38.4
Compulsory education + ≤ 1 year educ.	0.278	0.320	0.292	0.274	0.321	0.282
Upper secondary education ≤ 2 years	0.230	0.355	0.261	0.264	0.361	0.278
Upper secondary education ≤ 3 years	0.153	0.117	0.144	0.142	0.110	0.137
Short post secondary education	0.134	0.103	0.126	0.132	0.104	0.128
University education ≥ 3 years	0.205	0.105	0.180	0.188	0.104	0.175
# children at home	2.32	1.73	2.17	2.43	2.13	2.38
Union duration, years	7.33	3.87	6.46	10.6	7.28	10.12
(1) Parents married before child's birth[c]						0.585
(2) Married after birth[c] but before Fall 1989						0.184
(3) Married Nov-Dec 1989 – wife born ≥1945						0.077
(4) Married Nov-Dec 1989-- wife born <1945						0.001
(5) Cohabiting, partner born ≥1945						0.150
(6) Cohabiting, partner born <1945						0.003
# of observations	57,211	19,093	76,304	91,422	16,512	107,934

[a] The samples used in the cross-section analysis [b] In nominal SEK. [c] Refers to the child in the random sample, see Section 4.1.

Table 2b. Means and frequencies for the female samples[a] for 1985 and 1990 by marital status.

	Mothers 1985			Mothers 1990		
	Married	Cohabiting	All	Married	Cohabiting	All
Log earnings[b]	6.83 (.218)	6.78 (.174)	6.82 (.208)	7.23 (.246)	7.18 (.216)	7.22 (.242)
Age	33.6	30.2	32.7	37.0	34.3	36.6
Compulsory education + ≤ 1 year educ.	0.115	0.161	0.128	0.149	0.191	0.158
Upper secondary education ≤ 2 years	0.234	0.381	0.274	0.330	0.426	0.343
Upper secondary education ≤ 3 years	0.082	0.076	0.080	0.082	0.074	0.080
Short post secondary education	0.249	0.215	0.240	0.225	0.179	0.218
University education ≥ 3 years	0.320	0.167	0.278	0.214	0.130	0.201
# children at home	2.1	1.6	1.94	2.32	2.03	2.27
Union duration, years	6.7	3.4	5.8	10.9	7.6	10.4
(1) Parents married before child's birth[c]						0.607
(2) Married after birth[c] but before Fall 1989						0.173
(3) Married Nov-Dec 1989 – wife born ≥1945						0.070
(4) Married Nov-Dec 1989-- wife born <1945						0.001
(5) Cohabiting, partner born ≥1945						0.144
(6) Cohabiting, partner born <1945						0.004
# of observations	21,964	8,212	30,176	54,592	9,469	64,061

[a] The samples used in the cross-section analysis [b] In nominal SEK. [c] Refers to the child in the random sample, see Section 4.1.

Table 3a. Cross-section regressions on fathers' earnings in 1985.

Dependent variable: log annual earnings in 1985. Standard errors in parentheses. N=76,304.

	Model 1	Model 2	Model 3
Constant	Yes	Yes	Yes
Married	0.042	0.036	0.038
	(.002)	(.002)	(.002)
Dummies for level of education	Yes	Yes	Yes
Age and its square	Yes	Yes	Yes
Union duration		0.003	0.004
		(.0003)	(.0003)
# Children at home in 1985			-0.011
			(.001)
Adj R-sq.	.245	.246	.246

Note: Only fathers who earned more than nominal SEK 72,000 in 1985 are included. Average earnings for all fathers with non-zero earnings was SEK 163,000.

Table 3b. Cross-section regressions on mothers' earnings in 1985.

Dependent variable: log annual earnings in 1985. Standard errors in parentheses. N=30,176.

	Model 1	Model 2	Model 3
Constant	Yes	Yes	Yes
Married	0.0001	0.005	0.006
	(.003)	(.003)	(.003)
Dummies for level of education	Yes	Yes	Yes
Age and its square	Yes	Yes	Yes
Union duration		-0.002	-0.002
		(.0003)	(.0004)
# Children at home in 1985			-0.006
			(.002)
Adj R-sq.	.187	.188	.189

Note: Only mothers who earned more than nominal SEK 72,000 in 1985 are included. Average earnings for all mothers with non-zero earnings was SEK 62,300.

Table 4a. Cross-section regressions on fathers' earnings in 1990.

Dependent variable: log annual earnings in 1990. Standard errors in parentheses. N=107,934.

	Model 1	Model 2	Model 3	Model 4
Constant	Yes	Yes	Yes	Yes
Age and age squared	Yes	Yes	Yes	Yes
Dummies for level of education	Yes	Yes	Yes	Yes
Married	0.042 (.0024)			
(1) Ref. group: Parents married before child's birth	-	-	-	-
(2) Married after birth but before Fall 1989		-0.027 (.0023)	-0.024 (.0024)	-0.026 (.0024)
(3) Married Nov-Dec 1989 – wife born ≥1945		-0.046 (.0032)	-0.042 (.0033)	-0.045 (.0033)
(4) Married Nov-Dec 1989 – wife born <1945		-0.054 (.024)	-0.050 (.024)	-0.060 (.024)
(5) Cohabiting, partner born ≥1945		-0.054 (.003)	-0.049 (.003)	-0.054 (.003)
(6) Cohabiting, partner born <1945		-0.059 (.015)	-0.055 (.015)	-0.067 (.015)
Union duration			0.002 (.0002)	0.002 (.0002)
# Children at home				-0.20 (.001)
Adj R-sq.	.241	.243	.243	.246

[a] Refers to the child in the random sample, see Section 4.1.
Note: Only fathers who earned more than nominal SEK 100,000 in 1990 are included. Average earnings for all fathers with non-zero earnings was SEK 199,000.

Table 4b. Cross-section regressions on mothers' earnings in 1990.

Dependent variable: log annual earnings in 1990. Standard errors in parentheses. N=64,061.

	Model 1	Model 2	Model 3	Model 4
Constant	Yes	Yes	Yes	Yes
Age and age squared	Yes	Yes	Yes	Yes
Dummies for level of education	Yes	Yes	Yes	Yes
Married	0.001 (.002)			
(1) Ref. group: Married before child's birth [a]	-	-	-	-
(2) Married after birth [a] but before Fall 1989		-0.008 (.002)	-0.007 (.002)	-0.008 (.002)
(3) Married Nov-Dec 1989 – born ≥1945		-0.035 (.003)	-0.033 (.003)	-0.036 (.003)
(4) Married Nov-Dec 1989 – born <1945		-0.068 (.022)	-0.064 (.022)	-0.064 (.022)
(5) Cohabiting, born ≥1945		-0.006 (.003)	-0.004 (.003)	-0.008 (.003)
(6) Cohabiting, born <1945		-0.027 (.013)	-0.023 (.013)	-0.024 (.013)
Union duration			0.001 (.0002)	0.001 (.0003)
# Children at home				-0.014 (.001)
Adj R-sq.	.229	.231	.231	.233

[a] Refers to the child in the random sample, see Section 4.1.

Note: Only mothers who earned more than nominal SEK 100,000 in 1990 are included. Average earnings for all mothers with non-zero earnings was SEK 107,000.

**Table 5a. Fixed effects estimates on change in fathers' log earnings in 1985-90.
Standard errors in parentheses.**

	Model 1	Model 2
Constant	Yes	Yes
Married	0.013	0.005
	(.002)	(.002)
# Children at home		0.015
		(.001)
Time dummies	Yes	Yes
# observations	155,371	155,371
# groups	82,129	82,129
R-square within	.406	.408

Note: Earnings are measured in fixed 1990 prices. Only fathers who earned more than
SEK 100,000 (in 1990 prices) both years are included.

**Table 5b. Fixed effects estimates on change in mothers' log earnings in 1985-90.
Standard errors in parentheses.**

	Model 1	Model 2
Constant	Yes	Yes
Married	-0.058	-0.037
	(.004)	(.004)
# Children at home		-0.044
		(.002)
Time dummies	Yes	Yes
# observations	83,552	83,552
# groups	57,885	57,885
R-square within	.393	.409

Note: Earnings are measured in fixed 1990 prices. Only mothers who earned more than
SEK 100,000 (in 1990 prices) both years are included.

Table 6a. Fixed effects estimates on change in log earnings in 1985-90 for fathers who married in Nov-Dec 1989. Standard errors in parentheses.

	Model 1	Model 2
Constant	Yes	Yes
Married in Nov-Dec 1989	0.003 (.004)	0.003 (.004)
# Children at home		0.013 (.001)
Time dummies	Yes	Yes
# observations	25,940	25,940
# groups	13,984	13,984
R-square within	.422	.423

Note: Fathers who married in Nov-Dec 1989 are compared to those who continued to cohabit and who had a partner born in or after 1945, i.e., groups 3 + 4 are compared to group 5 in Figure 2. Earnings are measured in fixed 1990 prices. Only fathers who earned more than SEK 100,000 (in 1990 prices) both years are included.

Table 6b. Fixed effects estimates on change in log earnings in 1985-90 for mothers who married in Nov-Dec 1989. Standard errors in parentheses.

	Model 1	Model 2
Constant	Yes	Yes
Married in Nov-Dec 1989	-0.005 (.006)	-0.003 (.006)
# Children at home		-0.045 (.004)
Time dummies	Yes	Yes
# observations	14,643	14,643
# groups	10,330	10,330
R-square within	.315	.335

Note: Mothers who married in Nov-Dec 1989 are compared to those born in or after 1945 who continued to cohabit, i.e., groups 3 + 4 are compared to group 5 in Figure 2. Earnings are measured in fixed 1990 prices. Only mothers who earned more than SEK 100,000 (in 1990 prices) both years are included.

Table 7a. Fixed effects estimates on change in family log earnings in 1985-90. Standard errors in parentheses. (Fathers' earnings limit)

	Model 1	Model 2
Constant	Yes	Yes
Married	-0.157	-0.090
	(.009)	(.009)
# Children at home		-0.14
		(.001)
Time dummies	Yes	Yes
# observations	174,014	155,371
# groups	99,690	82,129
R-square within	.252	.264

Note: Earnings are measured in fixed 1990 prices. Only fathers who earned more than SEK 100,000 (in 1990 prices) both years are included.

Table 7b. Fixed effects estimates on change in family log earnings in 1985-90. Standard errors in parentheses. (Mothers' earnings limit)

	Model 1	Model 2
Constant	Yes	Yes
Married	-0.032	-0.026
	(.010)	(.011)
# Children at home		-0.013
		(.005)
Time dummies	Yes	Yes
# observations	89,156	89,156
# groups	63,615	63,615
R-square within	.293	.293

Note: Earnings are measured in fixed 1990 prices. Only mothers who earned more than SEK 100,000 (in 1990 prices) both years are included.

Table 8. Fixed effects estimates on change in log family earnings in 1985-90 for fathers who married in Nov-Dec 1989. Standard errors in parentheses.

	Model 1	Model 2
Constant	Yes	Yes
Married in Nov-Dec 1989	-0.006 (.015)	-0.001 (.015)
# Children at home		-0.16 (.001)
Time dummies	Yes	Yes
# observations	35,601	35,601
# groups	22,548	22,548
R-square within	.212	.228

Note: Fathers who married in Nov-Dec 1989 are compared to those who continued to cohabit and who had a partner born in or after 1945, i.e., groups 3 + 4 are compared to group 5 in Figure 2. Earnings are measured in fixed 1990 prices. Only fathers who earned more than SEK 100,000 (in 1990 prices) both years are included.